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ON THE EVIDENCE THAT THE EARTH'S INTERIOR IS SOLID.

BY DR. M. E. WADSWORTH.

(Continued from page 594, June number.)

The Evidence of the Earth's Solidity derived from the behavior of matter under the combined action of Heat and Pressure.—The key-note of this problem was given by Mr. William Hopkins in stating that if the tendency of the temperature to liquify the interior portion of the earth increases more rapidly than the pressure tends to solidify it, that interior would be in a state of greater or less perfect fluidity; but if the tendency to solidify from pressure is greater than the tendency of the temperature to prevent it, the earth would solidify from the center. In other words, whether the earth's interior is solid or not, depends upon the relative increase of the temperature and pressure, and on the behavior of the earth's materials under increased heat and pressure. Some experiments were made by Hopkins to ascertain the relation of solidity and fusion under the combined influence of heat and pressure on certain substances. He found that for spermaceti, wax, sulphur, and stearine as the pressure increased the point of fusion was raised, but irregularly, and with a diminished ratio, while metallic alloys showed no elevation of the fusion point. He did not regard his experiments as satisfactory, particularly those on the alloys.[1]

The subject was later discussed by Sir William Thomson, who held that from the "thermo-dynamic law of his brother, James Thomson, the earth must have solidified from the center outwards. This law asserts that all materials which contract on congelation have their melting point raised by pressure, while bodies that expand on freezing have their melting point lowered by pressure.

This law was experimentally verified by Wm. Thomson in the case of water. Hence, accepting Bischof's experiments as correct, which indicated that the earth's materials contracted from ten to twenty-five per cent on solidification, Thomson claimed that even if the internal heat was very great, the pressure would increase more rapidly than the tendency to liquify, and hence the earth must have a solid center.

The discussion then rests largely on the question of fact: Do the earth's materials contract or expand on solidification ? Mousson experimented further on the action of water according to James Thomson's law, and showed that it required enormous pressure to lower the melting point of ice a few degrees.[1]

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[1] Report Brit. Assoc., 1854, XXIV (Sect.), 57, 58; see also Bunsen, Ann. Physik Chemie, 1850, LXXXI, 562–567.

So far then as any conclusion can be drawn from these experiments, it would show that the pressure must increase to an enormous amount to keep pace with the supposed increase of heat—a pressure that the earth's specific gravity negatives.

It can well be claimed that the known rate of the elevation of temperature, as the interior is approached, far surpasses the known rate of change of the fusion point by pressure in the few cases investigated.

David Forbes has further pointed out that, since the substances mainly experimented upon by Hopkins and Bunsen are not, except sulphur, components of the earth, experiments made with them do not necessarily apply to the materials of which the earth is composed. He also suggested that if the pressure is raised to great heights the reverse may be true regarding the effect of pressure on the melting point—it may lower instead of raise it. He also thought that we should look rather at the results obtained by Hopkins from the alloys instead of those from the wax, etc.[2]

In discussing the state of the earth's interior, Dr. T. Sterry Hunt advanced the argument that the material of the earth, when in its former fused state, would solidify from the center on account of the congealed mass being much heavier than the liquid: " We may say in a few words that the process of cooling in a mass like this would be just like the cooling of a great bath of metal or of sulphur ; in other words, the condensation or congelation would commence at the center and extend outward toward the surface, so that the temperature of the center would therefore be the temperature of congelation."[3]

David Forbes, in reply to Dr. Hunt's argument, very pertinently remarked that no one " had ever seen a mass of metal or sulphur crystallize or solidify in the interior first, since the interior of such masses, it is well known, remain liquid after the crust has formed upon the surface; and furthermore, that the crust always remains upon the surface and does not sink."

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[1] Ann. Physik Chemie, 1858, CV, 161–174; Everett's Deschanel's Natural Philosophy, 1872, pp. 312, 313; 1883, pp. 331, 332.

[2] *Pop. Sci. Rev.*,1869, VIII, 121–130; *Geol. Mag.*, 1867 (1), IV, 431–444.

[3] *Geol. Mag.*, 1877 (1), IV, 357–369.

He also remarked that "a crust of the specific gravity of 2.65 cannot sink deep down into the fluid mass of a globe possessing the mean density of 5.3."[1]

In looking over the data for the assumed contraction of the materials forming such portions of the earth's crust as we are able to examine, it will be seen that the early experiments of Bischof indicated a contraction in passing from the liquid to the crystalline state for basalt of some ten per cent, of trachyte eighteen per cent, and of granite twenty-five per cent; or tabulating the results, we obtain, according to David Forbes:[2]

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	Fused.	*Glass.*	*Crystalline.*
Basalt	1000	963	896
Trachyte	1000	888	818
Granite	1000	888	748

The question of the contraction of igneous rocks in cooling, was further taken up by David Forbes,[3] although none of his observations appear to lead to any more definite conclusions than that Bischof's results were much too high, and that his work was too crude to be of any great value.

In a later paper Forbes pointed out that in nature there was no evidence of a contraction of from one-tenth to one-quarter of the volume, such as Bischof had held, when the rocks in the form of dikes, etc., were studied *in situ*—a statement that probably every petrologist will sustain.[4]

Some further experiments on the contraction of siliceous materials in cooling were made by Robert Mallet. He claimed that the result showed "that the difference in specific gravity, less than that between ice and water, between red-hot but solidified or even cold slag (or analogous fused rocks) and the same in liquid fusion, is so slight that, coupled with the viscous or pasty condition which intervenes between the two states, it would readily admit of a thin or a thick terrestrial solidified crust being supported by and upon the surface of the liquid globe beneath, and lends no support to the view of terrestrial consolidation at the center first, by continual subsidence of such crusts, as imagined by Poisson, nor to the notion as to the nature of volcanic action which Sir W. Thomson has based on that assumption. * * * We * * * may be permitted to conclude that rocks consisting of acid silicates contract still less than those of basic silicates, and that a terrestrial crust of the former is still more capable of floating upon the same in fusion beneath."[1] However much Mr. Mallet's views may agree with those of the present writer, it is but just to point to the fact that the slags experimented upon differ essentially from basaltic rocks, and it is not permissible to assume that one certainly gives the law for the other, however probable it may be that they act alike under similar conditions. This also applies in less degree to Mallet's comparison between plate glass and the rhyolites.

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[1] *Geol. Mag.*, 1867 (1), IV, 431–444; *Chemical News*, 1868, II (Amer. reprint), 110–113.

[2] Neues Jahr. Min., 1841, pp. 565, 567; 1843. pp. 1–54; *Chemical News*, 1868, XVIII, 191–194.

[3] *Chemical News*, 1868, XVIII, 191–194.

[4] *Geol. Mag.*, 1870 (1), VII, 1–4.

Further studies were made, in 1874, by Mr. Mallet on the alleged expansion of various substances on solidification. His experiment of filling a conical wrought iron vessel with molten iron seemed to show that liquid iron when heated far above its melting point has a specific gravity of 6.65, while the same iron in the cold state is 7.17. Mallet claims, therefore, that cold cast-iron is always heavier than molten iron. It is to be observed that no comparison was made between iron near the melting point and the same when just above that point.

He further tried some experiments regarding the flotation of solid iron upon molten iron, which must be regarded as conclusive upon that point. In all cases the iron either did not sink, or if when first immersed it sank, it afterwards rose again. Mallet says: "A piece of cold cast-iron which floats on liquid iron of its own quality, if forcibly thrust to the bottom and rapidly and at once released, rises again rapidly to the surface with all the *appearance* of a *buoyant* body, which it certainly cannot be."

The same experiments made with lead showed that it always sank when immersed in the molten lead. In this case when flat pieces were carefully laid upon the fluid lead they floated until the equilibrium was disturbed, so that the fluid could wet the upper surfaces when they sank. This flotation of the lead seems to be a case of the same kind as the flotation of a needle upon water. Mr. Mallet endeavored to explain the phenomena in the case of iron and lead by the assumption of a repulsive force, much the same kind of explanation as "Nature abhors a vacuum," for here the repulsion exists for iron but not for lead. He seems to have lost sight of the true problem before him: Is solid metal at or near the melting point of a greater or less specific gravity than the same metal liquid at or near the point of solidification, and substituted for it the question is cold solid metal heavier or lighter than the same metal when heated to *any* temperature above the melting point. Surely the tendency of most bodies to expand on the application of heat would, in the case of iron and rocks, cause the difference between the cold solid and the liquid states, to be greater than it would be between the hot solid and the liquid states, a point that does not seem to have been properly appreciated in discussions of the earth's structure. Mallet's experiments show clearly that iron near the point of fusion is lighter than the same when melted, but that lead is heavier. He calls attention to one very important point: That owing to the difference in specific gravity in the various grades of cast-iron, it is necessary that in these experiments the liquid and solid should be of the same quality, otherwise real buoyancy or the reverse may occur from this cause. Mallet further seems to admit that liquid slag will allow solid slag to float upon it, either on account of a certain vesicularity, whatever care may be taken, or on account of his hypothetical repellant force.[1]

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[1] Philos. Trans., 1872, pp. 147–227.

Some other experiments were made by Centner, W. J. Millar and Joseph Whitley.[2] The general result of these would be to show that cold steel, iron, brass and probably slag will sink in the same material when molten, but when hot and solid they become lighter and will float; also that lead and perhaps zinc possess a higher specific gravity when solid, whether hot or cold, than the molten metal.

In 1875 Mr. W. C. Roberts made some experiments by Mallet's method, on the density of silver in the cold state and in the molten condition. These showed that it was more dense in the former than in the latter state.[3]

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[1] Proc. Roy. Soc., 1874, XXII, 366–368; 1875, XXIII, 209–234; *Nature*, 1874, X, 156, 157.

[2] *Nature*, 1877, XV, 529, 530; XVI, 23, 24; 1878, XVIII, 397, 398, 464. Whitley's experiments are erroneously credited to Muirhead in Dana's Manual of Geology, 1880, 3d ed., p. 810.

[3] Proc. Roy. Soc., 1875, XXIII, 349, 350, 481–495.

Sir William Thomson in 1878 again took up the discussion of the earth's solidity, based on the assumed greater density of the solid than liquid rock. He seems at that time to have only been aware of Bischof's experiments, but in a note subsequently added by him, Whitley's experiments appear to be accepted as decisive against his views. Starting with the, to him, probable supposition that the solidified crust of the earth would sink towards the center, Thomson says: "As soon as the surface began to freeze, and to freeze in sufficient quantity not to be floated up by mere superficial solidified foam, the mass of rock would fall down towards the center. More would then solidify at the surface. This also would fall down, and the same thing would go on again and again. Gradually a sort of honey-combed solid would be formed. By-and-bye [*sic*] a skeleton or frame-work through the whole would mount up to an extent sufficient to build up piers, as it were, to the surface, and the spaces between these piers, when close enough, would, in the continued freezing of the lava, be bridged across by solid rock thick enough in proportion to breadth not to break down and sink. There would, again, be breaking away of the piers and upheavals of the liquid material below; but by degrees the honey-combed mass would become nearly like a solid throughout with comparatively small interstices of liquid lava. * * * The conclusion to be drawn respecting the internal condition of the earth is, that we are not to infer liquidity of the interior, even if we should find evidence of a much higher internal temperature than that which would melt the rocks under ordinary pressure."

The interior heat, Thomson states, "may be 4000° F., or 5000°. It may possibly be 8000° or 10,000°."[1]

In 1879 investigations were made by Messrs. J. B. Hannay and Robert Anderson on the expansion of cast-iron when solidifying. The chief method used was the flotation of a sphere of cast iron in a molten bath of the same. The latter was cooled near to the solidifying point and then the solid spheres dropped in. "They were found to sink at once when dropped in cold, and they remained under the metal till they had acquired a temperature just approaching visible red; but at that temperature they rose to the surface, and as they gained more and more heat from the liquid metal, their line of flotation rose higher and higher. Sometimes, if dropped in suddenly, the spheres did not float until they had begun to melt, but this was owing to their having cemented themselves to the bottom of the pot. When dropped in cautiously, or suspended by a wire, they sank only for the space of twenty to twenty-five seconds, and rose to the surface when barely red hot."

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[1] Trans. Geol. Soc. Glasgow, 1878, VI, 38–49.

These experimenters found that liquid cast-iron expands at least 5.62 per cent of its volume in solidifying. A result that they regarded as under the truth, since the maximum density of the molten iron seemed to be little if any above the melting point, while they were obliged to use it at a temperature above the point of maximum density.[1]

Experiments upon the relative volumes, solid and liquid, of tin, lead, zinc, bismuth, cadmium, antimony, iron and copper have been made by Nies and Winkelmann. These show that tin, zinc, bismuth, antimony, iron and copper in passing from the fluid to the solid state expand; that is, if both the solid and liquid densities of the metals are compared when both are at a temperature near the melting point, the solid has the less specific gravity. Hence these metals when hot and solid would float on the liquid, and pressure, according to Thomson's law, would lower their fusion points, thus they would remain liquid at a lower temperature in the earth's interior than on its surface,[2] a fact which may assist in explaining the eruptive origin of some iron ores.[3]

Further experiments by Messrs. Roberts and Wrightson on bismuth and iron showed that "iron expands rapidly (as much as six per cent) in cooling from the liquid to the plastic state, and then contracts seven per cent to solidity, whereas bismuth appears to expand in cooling from the liquid to the solid state about 2.35 per cent.[4]

From the above detailed experiments and observations it is rendered quite clear that the chief portion of the metals expand in passing from the liquid to the hot solid state, both having their density taken near the melting point. That this is the case with the rocks, also, there seems but little doubt, although further careful experiments are needed. Practically this seems to be the case in nature of the Kilauea lavas at least.

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[1] Proc. Roy. Soc. Edin., 1879, x, 359–362.

[2] Sitz. Akad. München, 1881, pp. 63–112.

[3] Proc. Bost. Soc. Nat. Hist., 1880, xx, 470–479.

[4] *Phil. Mag.*, 1881 (5), xI, 295–299.

The question which now presents itself is: What are the probable components of the earth's interior?

So far as can be told from petrographical study, it appears probable that the portion of the interior mass lying nearest the center, concerning which we have any data, is composed of iron[1] either with or without nickel. As we recede from the central portions pyrrhotite is united with the nickel and iron. Then these minerals are found united with olivine or olivine and enstatite in varying proportions, until a portion is reached composed almost entirely of one or both of these silicates, with or without diallage. This portion passes on into the common basaltic rocks, then into the andesites and so on outward into the trachytic, rhyolitic, and jaspilitic forms.

However true this order may be for the liquid earth, that is, the liquid material would form rocks of this character, it is certain that in the solid portions these materials are interlaced now with one another in every conceivable way, and in the chemical and sedimentary deposits they have now been intimately mingled.

What may be the composition of the earth's mass nearer the center, if there be any, besides the iron and nickel, we have no clue, except that it may be some of the rarer elements now found mixed with the iron.

Now, while we know experimentally almost nothing of the behavior of such materials, as probably compose the earth's interior, under the combined action of heat and pressure, it seems most probable, from Thomson's "thermo-dynamic law" and the experiments on their relative hot solid and liquid densities, that the pressure to which they are subject would cause them to liquify at a lower temperature in the earth's interior than on its surface. It may also be justly claimed that if the earth's interior is solid, its liquefaction may be brought about by an increase instead of a diminution of pressure. In this way a sinking area loaded with sediments might thus liquify the rock beneath it. Is this not as consistent a view as the theory of liquefaction through the removal of pressure?

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[1] Whitney's Metallic Wealth of the United States, 1854, p. 434, Judd's Volcanoes, 1881, pp. 307-324.

The best test of the relations in density between molten and solidified rock is apparently to compare the density of the rock just before fusion, or at least near that point, with the density of the same rock after melting. This would give a comparison between the crystalline and liquid states, while the usual method only affords a comparison between the liquid and the glassy or semi-glassy states. It would also save any error arising from cells in the cooled rock, if a solid mass was chosen in the first place. Again the fresh unaltered varieties of a rock should be chosen instead of such old and altered ones as those usually experimented upon.

In all discussions relating to the question of the liquidity of the earth's interior, it is to be borne in mind that the chief portion of our knowledge of the properties of liquids is derived from the study of water, a mobile liquid—while liquid rock, as lava or melted iron, is viscous, and its laws and properties may on experiment be found to differ considerably from those of water, under like conditions. Also in these solids the passage from the solid to the liquid state or the reverse is not abrupt as is the case with water, for every grade of viscosity exists between the normal solidity and the approximately perfect liquid condition. This is especially the case with iron and seems to be so for the common rocks.

(*To be continued.*)

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THE TERTIARY MARSUPIALIA.

BY E. D. COPE.

SINCE Cuvier discovered an opossum in the gypsum of Paris, the knowledge of the Marsupials of the Tertiary periods of Europe and North America has been gradually extended. In Europe they have been traced to the Middle Miocene, when they disappear from that continent. In North America we know them from Oligocene beds (White River), when they disappear, and are only known as yet thereafter as members of the existing fauna. Descending the scale we have them in the Laramie in America and Jurassic in America and Europe, and in the Trias in South Africa. Whether the Triassic Mammalia of the northern hemisphere belong to this order or not is uncertain. Under the head of Creodonta[1] I have discussed the marsupial resemblances of

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[1] NATURALIST, March, April and May, 1884.